DISCUSSION BEFORE THE WIRELESS SECTION, 3RD FEBRUARY, 1932.

Mr. E. B. Moullin: With regard to formula (9), the purpose of this formula is to show how to deal with any charge distribution, and also to show that distance should be reckoned from the top of the aerial. When r_1 is put equal to r_0, formula (9) becomes the same as formula (3*a*), and hence we conclude that the doublet formula can be used, provided the effective height is reckoned in the correct manner. I think it is worth while to look at this question from a slightly different point of view. We may suppose the aerial to be built up of a series of doublets of different lengths, placed, not end to end, but on a straight line with a common middle point. The charge for any particular doublet of length l is the charge to be found on the aerial at a height $\frac{1}{2}l$ above the middle point, and the current corresponds to these charges. For points very distant compared with the height of the aerial, the fields due to all these doublets can be added algebraically. Hence it follows that the resultant field depends upon $\int idl$, which is what the authors have proved. This argument, which is as exact as, and more direct than, that leading to formula (9), is given on pages 19, 417, and 421 of my book "Radio Frequency Measurements." With respect to the use of r_1 instead of r_0 in formula (9), I doubt whether this formula is quite as exact as the authors think, although the discrepancy is much less than could be detected experimentally. We can see at once that the important term must depend on $1/r_1^3$, for very close up to the aerial the field is sensibly that of the same static charge. Formulæ (43) and (45) of my book give the field close up to a dipole, and thereabouts the field is sensibly the same as if r_1 were substituted for r_0 in the doublet formula; but it is not quite the same. In Fig. 10 I believe that Er_1^3 should be constant, and I am sure that the very small discrepancies are due only to some little error of measurement. I am glad to hear that the system of current division by parallel condensers was successful. I think this is one of the first recorded uses of the principle, which is now much under discussion for standardization processes. It is very satisfactory to find that the supposed discrepancies in the re-radiation problem have now been finally removed. I am particularly interested in Section (5), which at last shows experimentally that the field of an aerial is that due to the aerial and its image. I only wish I could derive an analytical proof which would satisfy my physical picture of the mechanism of the process. I dislike intensely the usual reflection argument; it may be sound, but I cannot reconcile it with the basic conceptions of electromagnetic theory. It remains for some mathematician to show that the field due to the charges on the ground is the same as the field of the image. The problem involves integration over the whole equatorial plane, and up to the present I have found it intractable. I have made some progress towards a solution in that I can show that the charges in the vicinity of the base of the receiver aerial suffice to make the necessary increment, but so far I have not succeeded in showing that the charges all over the remainder of the infinite plane produce a vanishingly small resultant. I hope the authors will explain why suppositions (*b*) and (*c*) of Section (5) give an incorrect result. It is a great achievement to have obtained the experimental confirmation of the doublet formulæ which is shown in Figs. 7 and 9.

Prof. E. Mallett: The paper removes any doubts about the application of the usual mathematical theory of the propagation of electromagnetic waves that may have arisen from the work of other experimenters. A

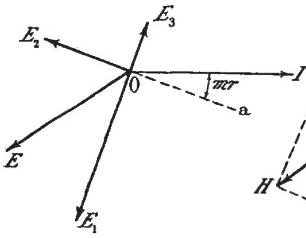

FIG. A.

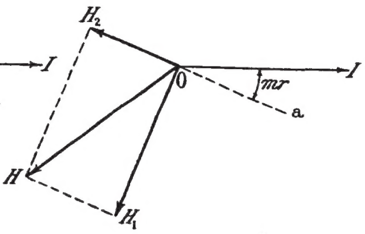

FIG. B.

vector statement of equations (3*a*) and (3*b*) is illuminating. It is well known that at a great distance from an aerial the electric field and the magnetic field are in phase. Although equations (3*a*) and (3*b*) make it appear as if they were in phase opposition, this is only a question of the particular choice of the positive direction. By the opposite choice the equations may be rewritten

$$E = E_1 \sin(pt - mr) - E_2 \cos(pt - mr) - E_3 \sin(pt - mr) \quad (3a)$$

$$H = H_1 \sin(pt - mr) - H_2 \cos(pt - mr) \quad . \quad (3b)$$

Here $\quad E_1 : E_2 : E_3 :: 2\pi/\lambda r : 1/r^2 : \lambda/2\pi r^3$

and $\quad H_1 : H_2 :: 2\pi/\lambda r : 1/r^2$.

When $r = \lambda/2\pi$, $E_1 = E_2 = E_3$, and $H_1 = H_2$. These fields arise from an aerial current $I \cos pt$, and in the vector diagrams of Figs. A and B the horizontal projections are taken for instantaneous values and OI (the aerial current) is taken as the reference vector. Then if (Fig. A) Oa is drawn below OI at an angle mr, OE_1 is at right angles behind Oa, OE_2 along aO produced, and OE_3 along E_1O produced, then OE_1, OE_2, and OE_3 represent the three terms of equation (3*a*)

taken in order. Similarly in Fig. B, OH_1 and OH_2 represent the two terms of equation (3b). The resultant fields OE and OH are found by vector addition in the usual way. As the distance r is increased the components of the fields rotate in a clockwise direction (as well as being modified in magnitude), and the projections on the horizontal axes of the diagrams give the instantaneous values. The authors' shadow-aerial experiments are particularly interesting, and it is simple from the engineer's point of view to regard them as a vector problem. Referring to Fig. 12, let OV in Fig. C represent the field at the aerial A or (by a change of scale) the e.m.f. in A. As the capacitance C of the aerial condenser is varied the locus of the aerial impedance will be given by the vertical straight line aRb drawn at a distance OR from O equal to the aerial resistance, and traversed upwards as C is increased. The aerial current in A will be the inversion of this line with a suitable factor, i.e. the circle OfD described clockwise as C is increased. This current causes a field at the aerial B at a distance r; in the experiments r was so small that

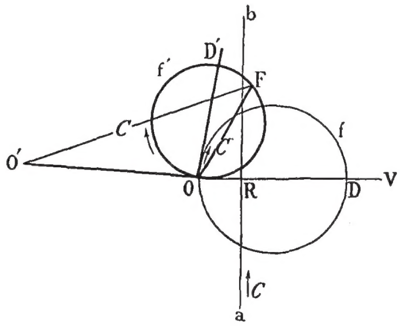

FIG. C.

the component E_3 was chiefly effective in producing currents in B. To give the electric field at B due to the currents in A, therefore, the circle is rotated counter-clockwise, through nearly a right angle (see Fig. A) and suitably altered in magnitude. The circle so obtained is Of'D'. The field due to the original transmission is now added as O'O, drawn practically in phase with OV as the distance r is so small. The total field is OF, i.e. the vector sum of O'O and the rays drawn from O to the circle Of'D', and as C increases its magnitude varies in the manner indicated by the experimental results of Fig. 14. The curve obtained by plotting the values of the total field is very sensitive to small changes of capacitance near resonance, and I suggest that this method would be a useful one to employ for detecting the presence of hidden aerials. I am more fortunate than Mr. Moullin in the matter of the image. I have always accepted without any query the fact that if a conducting plane is placed midway between the extremities of a dipole the field will not be affected in any way. If a conducting plane is placed between a pair of telegraph wires it should not affect the fields in any way, and I therefore do not see why a similar argument should not be true for the dipole, provided one has a perfectly conducting earth. The authors state that there does not at present appear to be any theory which shows clearly why the factor β should be equal to 2, but it seems to me that such a theory would be called upon to show why β should not be equal to 2.

Prof. L. S. Palmer: The first pages of the paper lead one to expect some description of a better method of dealing with those problems of short-wave reflection to which reference is made in Section (2). On the contrary, the paper supports the approximations and the methods of treatment which have been employed, both in this country and in America, for dealing with short-wave problems in which the value of r/λ is essentially greater than $0 \cdot 2$. In a previous paper* Mr. Honeyball and I referred to E (shown by the full-line curve in Fig. 3) as being essentially a monotonic function which is practically independent of r/λ for the critical values of this quantity ($0 \cdot 33$ and $0 \cdot 85$) with which I have been concerned, and on which Englund and Crawford have worked in America. I am therefore gratified to find that the authors are able (see Figs. 5 and 6) to justify the assumption that for distances greater than $0 \cdot 2\lambda$ one may apply the dipole theory to $\frac{1}{2}\lambda$ aerials which are not essentially of the dipole form. In my opinion the "previous measurements" referred to in Section (2) are really not relevant to what comes after, except in so far as the authors' theory supports the approximations on which they were based. The justification for using the dipole theory for these relatively large values of r/λ is even more marked in connection with frame aerials, where the current distribution is much more uniform. The measured critical values of the frame width and height agree very closely with the values calculated on the assumption that the side of the frame acts as a Hertzian dipole, even though the length of the side may be an appreciable fraction of the wavelength. With regard to the question of re-radiation from an aerial (Section 4), three points of interest arise. The first concerns the curves shown in Fig. 14; the second is the consequence of neglecting the phase angle η, due to the distance between the two aerials; and the third is the value for h' used in calculating the values of a shown in the table on page 534. Concerning the first point; if we differentiate with regard to θ the expression for ${}_0E_B^2$ in equation (22) and equate to zero, we can determine the positions of the maxima and minima of the curves shown in Fig. 14. The results are as follows: Fig. 14(a), a minimum at $\theta = 57°$ and a maximum at $\theta = -33°$. For Fig. 14(b) the corresponding figures are 48° and $-$ 42° respectively, and for Fig. 14(c) they are $46 \cdot 5°$ and $- 43 \cdot 5°$ respectively. These values, which are based on the assumption that Figs. 14(a), (b), and (c) correspond to Figs. 15(a), (b), and (c) respectively, imply that the signal-voltage minima shown in the full-line curves of Fig. 14 should lie further than the maxima from the peaks of the curves indicated by means of crosses. These peaks occur where $\theta = 0$, and the signal-voltage curve should move to the right as a decreases. The foregoing conclusions are not obvious from Fig. 14, because, in the first place, the full-line theoretical curve is made to coincide with the experimental values by taking the best circle that will fit the observed points in Fig. 15; and, in the second place, no scales of abscissæ are shown on the graphs. If such scales (preferably in values of $\pm \theta$)

* L. S. PALMER and L. L. K. HONEYBALL: "The Action of a Reflecting Antenna," *Journal I.E.E.*, 1929, vol. 67, p. 1045.

were added to Fig. 14 one would be able to see whether or not the experimental maxima and minima agree with the values calculated from equation (22). Such agreement will also depend on the angle η which has been neglected, and on whether aerial B was tuned or not. I presume it was, although this is not stated. I am more interested in the variation of the field with r than with θ, especially for values of r equal to about $0 \cdot 16\lambda$. At this value the phase angle ϕ, which has been taken throughout to be 90°, becomes zero. The authors have worked at such short distances that they have missed all the interesting points, which depend upon the angles ϕ and η. The second point which requires consideration is the phase angle η. This angle, determined by the aerial spacing, has been neglected throughout, although it is pointed out on page 536 that this is not permissible for values of θ given by OC in Fig. 15, i.e. for values of θ corresponding to the minimum points in Fig. 14. However, η is also not negligible when the aerials are tuned and $\theta = 0$, because for this condition the value of η decides whether the current in the aerial B shall be greater or less than that in A. This follows from the fact that equation (22) becomes

$$_0E_B^2 = E_0^2[1 + a(a - 2 \sin \eta)]$$

when $\theta = 0$, and η is not neglected. Thus for a distance of 15 metres between A and B the current in B is less than that in A, whilst for a distance of 10 metres the reverse is the case when A and B have about the same ohmic resistance. This seems to be an important consideration when checking the ordinates of the curves in Fig. 14. It is also a point of considerable practical interest when using Lecher wires. I should be grateful for further information about the value of h' used in calculating the values of a in the table on page 534. Assuming that the values in the first column refer to r_0 and that h is 4 metres (the value mentioned in parentheses on page 535), the value of r_1 can be calculated and substituted in the formula for a on page 533. This leads to values of h' which increase from $3 \cdot 02$ to $3 \cdot 86$ metres as a decreases from $0 \cdot 88$ to $0 \cdot 10$. Is this variation due to a change in the current distribution in the aerial A? I think this question is of some importance because the shape of the curves in Fig. 14 and the position of the minima and maxima obviously depend on the value of a, and therefore on h'. There is, however, the alternative suggestion that the resistance of the aerial changed. Although it is impossible to apply the Hertzian dipole theory to explain the present measurements with values of r/λ less than $0 \cdot 015$, unless the aerial used approximates to a dipole, yet it is gratifying to find that the authors' conclusions justify the use of the dipole theory for values of r/λ greater than $0 \cdot 2$ even when the aerial does not approximate to a Hertzian dipole. This conclusion was reached in 1929, when the experimental data published in the *Journal** were found to be within 5 per cent of the values calculated from the ordinary dipole theory. For values for r/λ greater than $0 \cdot 7$ the theoretical and experimental results did not differ by more than 3 per cent. It is pleasing to me to find that a problem which was under consideration at Manchester in 1927 has now produced such valuable results at Cambridge.

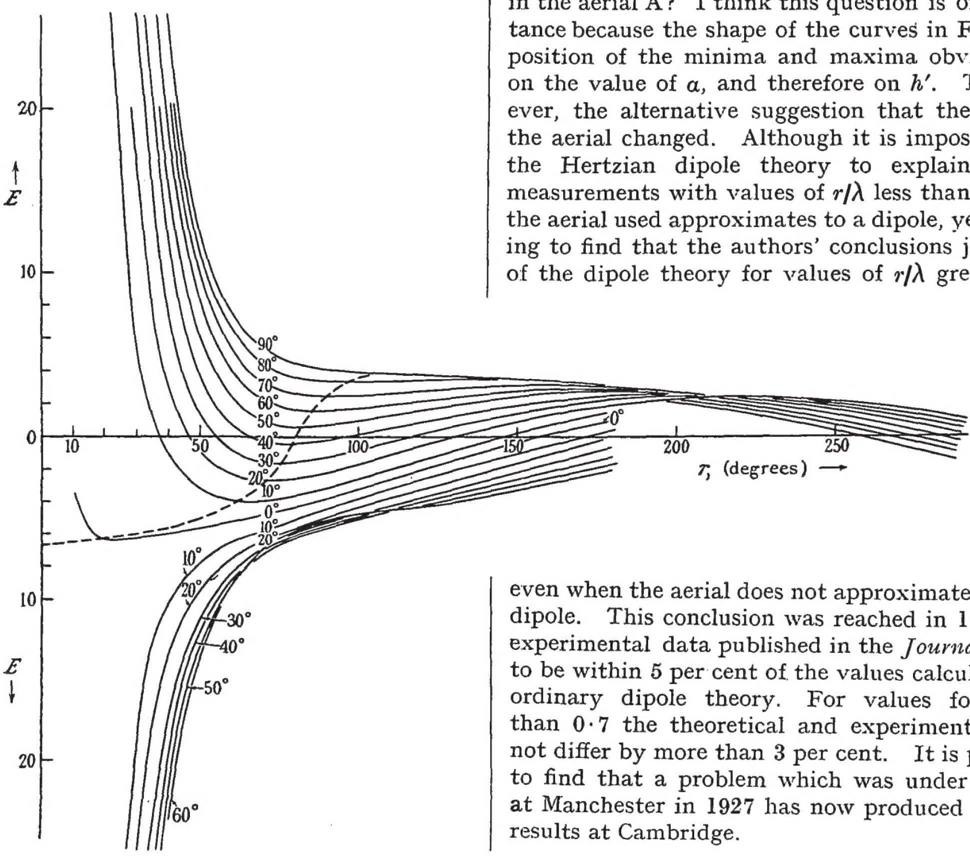

FIG. D.—Field close to a Hertzian dipole oscillator.

(*Communicated*): The difficulty of picturing the anomalous nature of the field in the neighbourhood of a radiating Hertzian-dipole oscillator seems to be greatly reduced by considering the curves shown in Fig. D, which were calculated and drawn by Mr. W. Anderson of the Manchester College of Technology. They are "iso-phase" graphs of the general Hertzian dipole formula

$$E = E_0\left[\frac{1}{r^3}\cos\omega\left(t - \frac{r}{v}\right) + \frac{1}{vr^2}\frac{d}{dt}\left\{\cos\omega\left(t - \frac{r}{v}\right)\right\} + \frac{1}{v^2r}\frac{d^2}{dt^2}\left\{\cos\omega\left(t - \frac{r}{v}\right)\right\}\right]$$

and are plotted for phase differences from $- 60°$ to $+ 90°$.

* *Loc. cit.*

The ordinates are field values and the abscissæ values of the distance r in degrees, one wavelength being equivalent to 360°. The intersections of the curves on any line $r = x$ show how the field varies with time both in magnitude and in phase at any given distance x from the dipole. The dotted curve is the locus of the field minima, and the curious way in which it changes from negative to positive values at $r = 80°$ (or $r/\lambda = 0 \cdot 22$) is clearly indicated. The velocity of the change in the direction of wave propagation is quite different from the wave velocity. The curves also show the origin of the idea of " loops of radiation " breaking away as the crossing-over of the lines of force takes place during the oscillatory motion of the charges along the axis of ordinates.

Mr. J. S. McPetrie: Mathematical physicists agree that most of the radiation from an aerial takes place from the free end. The results deduced by the authors for the field near an aerial would follow immediately without proof. Their analysis, however, is interesting as it is made from a more strictly physical point of view. The effect of the earth has been neglected in obtaining the theoretical curves of Figs. 7 and 9. Would the agreement between the theoretical and experimental results shown in these figures have been less striking if the reflection coefficient at the earth's surface had been different from unity? The magnetic field at any point in an electromagnetic wave can always be obtained from the electric field at that point by an equation of the type $\partial H/\partial t = \text{curl } E$. I suggest, therefore, that once agreement had been obtained between theory and experiment (see Fig. 7) as regards the value of the electric field, there was no real necessity for measuring the variation with distance of the magnetic field—although it emphasizes the experimental accuracy. On page 535 the authors endeavour to use Fresnel's equations for the reflection coefficient at the earth's surface when the receiver is very near the ground. Fresnel's equations cannot be used under this condition because the reflected field considered in these equations is plane. This means that for the equations to apply the distance of the receiver from the reflecting surface must be such that the field re-radiated by the oscillators in that surface is plane before it reaches the receiver. When the receiving aerial is actually earthed at its lower end this condition is not satisfied, so that Fresnel's equations cannot be used. It is interesting to note, however, that Fresnel's equations give the " radiation " component of the reflected field; they assume a plane reflected wave which, therefore, contains only the radiation component. As the authors point out, this is opposite in phase to the incident wave. When the receiving aerial is close to the earth the predominant component of the reflected field is what is commonly termed the " static " component. This component, which is in phase opposition to the " radiation " component, accounts for the fact that there is appreciable radiation directly along the surface of the earth.

Mr. F. W. G. White: The authors show that the electric field at any point in space about an aerial in which the current distribution may be represented by a portion of a sine curve, can be calculated by equation (4). In almost every case met with in practice the current distribution is substantially sinusoidal, and we may therefore use this equation to calculate the electric field of most aerials at all positions in space, even at points very close to the aerial itself. The equation may be used not only for straight aerials but also for aerials with a horizontal portion, if the current falls to zero at the free end in a sinusoidal manner; we may think of an inverted L aerial, for example, as a straight aerial with its end portion bent into the horizontal position. The calculation of the electric field at a large distance from an inverted L aerial erected over perfectly conducting ground, has been carried out in the following way. We assume that the length above the ground is $\frac{1}{4}\lambda$, the length of the up lead and of the horizontal portion being $\frac{1}{8}\lambda$, while the total length including the image is $\frac{1}{2}\lambda$. Let r be the distance from the earthing point to the distant point. We can divide the straight aerial, before we bend it to form an inverted L, into four portions, and express the electric field parallel to the wire by means of the equation

$$\begin{aligned} E = (i_0/r)\big[&\cos(k\,.\,\lambda/8)\cos(pt-kr_2) - \cos(pt-kr_1) \\ &+\cos(k\,.\,2\lambda/8)\cos(pt-kr) - \cos(k\,.\,\lambda/8)\cos(pt-kr_2) \\ &+\cos(k\,.\,3\lambda/8)\cos(pt-kr_3) - \cos(k\,.\,2\lambda/8)\cos(pt-kr) \\ &+\cos(k\,.\,4\lambda/8)\cos(pt-kr_4) - \cos(k\,.\,3\lambda/8)\cos(pt-kr_3)\big] \end{aligned}$$

since we must take into account the variation of phase with distance. We now bend the top portion ($\frac{1}{8}\lambda$) into the horizontal to form the inverted L, and the bottom ($\frac{1}{8}\lambda$) into the horizontal to form its image, and substitute the correct values of r_1, r_2, r_3, and r_4, in terms of the distance to the centre of the aerial r. This gives

$$\begin{aligned} E = (2i_0/r)\Big[&\cos(pt-kr)\Big\{-\sin(\tfrac{1}{4}\pi\cos\theta)\sin(\tfrac{1}{4}\pi\sin\theta) \\ &\sin\theta - 0\cdot707\cos(\tfrac{1}{4}\pi\sin\theta)\cos\theta\Big\} \\ &+\sin(pt-kr)\Big\{-\cos(\tfrac{1}{4}\pi\cos\theta)\sin(\tfrac{1}{4}\pi\sin\theta) \\ &\sin\theta + 0\cdot707\sin(\tfrac{1}{4}\pi\sin\theta)\sin\theta\Big\}\Big] \end{aligned}$$

This is the field perpendicular to the radius vector joining the centre of the aerial to the distant point. θ is the angle made by the radius vector and the horizontal.

Mr. R. H. Barfield: The part of the paper which is most interesting to me is that which deals with the shadow-throwing properties of receiving aerials when they are in or near resonance and the transmitted wave is passing over them. The authors have shown conclusively that under " ideal " conditions the experimentally-determined resultant field in the shadow region of a single aerial agrees with a value calculated on the simple Hertzian dipole theory. The experiments* by Mr. Munro and myself to which the authors refer, showed that the shadow thrown by a group of aerials in resonance with the transmitted wave was of less depth than that thrown when the aerials were just off resonance. This was a puzzling result, first because common sense would argue that maximum shadow should correspond to maximum energy absorption, i.e. to resonance conditions, and secondly because theory based on re-radiation

* *Journal I.E.E.*, 1929, vol. 67, p. 253.

calculations showed that resonance should—at points in the direct line—give the opposite effect to a shadow, i.e. an actual increase over the normal value. We were thus faced with a state of affairs in which common sense was in conflict with theory, while the experimental results were in conflict with both. We therefore decided to make some further experiments with short waves and small model aerials, thus getting much simpler and more controllable experimental conditions. The most interesting result of these experiments was that shadow curves for a single aerial were obtained similar to those shown in Fig. 14, but they differed from theory in exactly the same way as those obtained in the earlier experiments on broadcasting waves. We could think of no explanation which would fit both cases, other than that referred to in the present paper. Since then, however, another and more satisfactory explanation for the short-wave case has been found by J. S. McPetrie,* namely that the phase shift is due to the imperfection of the image in the earth of the transmitting aerial. The possibility of this explanation had been considered by us previously, but had been rejected for the reason that it could not explain both cases. McPetrie's work puts the validity of the explanation practically beyond doubt, however, and we have in consequence been seeking an explanation of the earlier full-scale broadcasting shadow experiments. Such an explanation is obtained by considering the effect of the aerials flanking the line joining the transmitter and the receiver (see Fig. E). The energy passing from the transmitter to the intermediate (or shadow-throwing) aerials and thence in the form of re-radiation to the receiver, has a " path-difference " from the energy which travels direct. This introduces a phase lag between components arriving from the flanking aerials and those arriving from the aerials intervening in the direct line. Such a lag will cause the shadow curves to be shifted sideways in the right direction, and certainly of sufficient magnitude to reconcile theory and experiment. There still remains the task of reconciling theory and experiment on the one hand, and common sense on the other. Why should not maximum shadow be produced at resonance? This difficulty also disappears on close consideration when it is remembered that in optics a good reflector may cast a perfect shadow while only absorbing a negligible amount of the incident energy. In other words, reflection or scattering may be relied upon to deviate the energy from a given spot to an extent entirely independent of energy absorption.

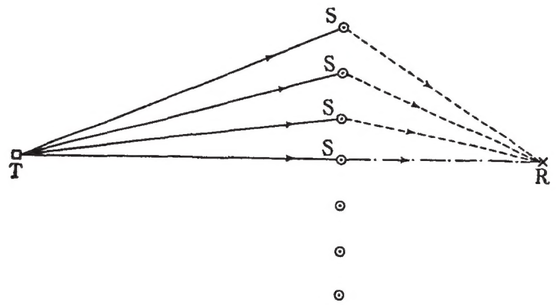

FIG. E.

* *Journal I.E.E.*, 1932, vol. 70, p. 382.

Dr. R. L. Smith-Rose: The paper deals essentially with the experimental verification of the equations giving the electric field due to an aerial, and this verification has been carried out at a shorter distance from the aerial than has been adopted by previous workers. The authors were not led to examine the validity of the equation because someone had disputed it, and in this sense I suggest that they have been a little too critical of the work of previous experimenters. The objects of those workers were somewhat different, and so far as the nature of their experiments was concerned—particularly for the short waves with which they were working—the agreement they obtained between experiment and theory was adequate for their own purposes. There has, however, been some question as to the validity of the equations in the immediate neighbourhood of the aerial, and I am glad to note that Prof. Mallett and Mr. Moullin are now very clear upon the point. The teachers of the generation of students to which I belong rather tended to treat with an air of mystery the phenomena occurring in the immediate neighbourhood of the aerial. The object of wireless transmission is communication over long distances, and it was not until we had become concerned with short waves that we began to take an interest in the state of affairs a little closer to the aerial. The authors apparently thought that some explanation was necessary for the fact that they measured the magnetic field separately from the electric field. In my opinion, however, their explanation is not quite complete. The magnetic field is nothing more or less than a change or movement of the electric field, and we must be able to interpret the one in the terms of the other. The value of E to which the authors refer throughout is the vertical component of the electric field. When we use a loop we do not measure the vertical electric field only, and so the effect obtained with the loop may not always be the same as with the aerial. The two curves in Fig. 3 show that close up to the aerial the electric field appears to be stronger than the magnetic field. This is entirely due to the fact that the electric field at such a short distance from the aerial is not vertical. This explanation may clear up the rather unsatisfactory footnote on page 528, in which the authors draw attention to the fact that the equivalent E deduced in the way they have just explained is not the actual value of E at the point considered, since they were working inside the first wavelength, where the usual relations for the radiation field did not hold. It is purely a question of which components of the electric field are being considered.

Mr. L. B. Turner: I am surprised that throughout the paper and discussion no reference seems to have been made to the effect of the flat top of the aerial. In calculating the field the only justification for neglecting the currents in the flat top is, I presume, that at distances large compared with its width the field set up by any one element of current is sensibly cancelled by the field from a corresponding opposite element. I do not think this argument holds, however, for measurements at such extremely short distances as those shown in Fig. 10, where the flat top has the effect which possibly ought to have been taken into consideration.

Dr. E. H. Rayner: If the effect of the flat top

of the aerial can be neglected the results ought not to be affected by replacing the authors' aerial by a T aerial with its plane either in the direction of propagation or at right angles to it. One might expect a zero effect at right angles, while in the direction of propagation there might be an appreciable component which would possibly have some influence on the received signal.

Dr. Mary Taylor : In their paragraph on the variation of field with distance, the authors make no reference to the fact that the field they are measuring is not that due to a dipole in free space but is produced by the superposition on such a field of the disturbance caused by the presence of the earth. The only indication that this has been considered is the remark (regarding the third division of the experimental work) that the height of the transmitting dipole with its "image" in the ground is 8 metres. The fact that experimental and theoretical values for E and H can be made to fit so closely (Figs. 7 and 9) really demonstrates the validity of the image theory of the effect of the earth in the range of frequencies chosen by the authors. If, instead of fitting the experimental and theoretical values for E and H shown in Figs. 7 and 9, they had compared the calculated and measured values they actually obtained, they would have found that the latter were greater than the former by the factor 2 which they state (page 536) has hitherto been unexplained by theory. As Sommerfeld has shown, the theory of propagation of electromagnetic waves from a vertical dipole on a plane earth (from which the image theory is deduced) predicts this value for the case of a perfectly conducting earth.* The authors' experimental results show that the earth may confidently be regarded as a perfect conductor at the frequencies they have chosen to employ.

Mr. T. L. Eckersley (*communicated*): I am rather at a loss to know what was the exact aim of the authors' investigations. At this stage in the history of electromagnetism it is hardly possible to doubt the validity of Maxwell's equations and their application in such a case as that dealt with in the paper. If, for instance, the experiments had not yielded results in accordance with the theory, I would have trusted the theory rather than the experiments and would have asked myself in what respect the experimental conditions differed from the ideal ones considered in the theory. The ideal case is that of a Hertzian oscillator cut through the middle by a perfectly conducting plane, and the experiments seem to me to prove that in the case under consideration the effect of the earth approximates to that of such a plane. The conditions chosen approach the ideal very closely. Theoretically this should occur when

$$2\sigma\lambda c >> 1$$

where σ is the earth conductivity in electromagnetic units, λ is the wavelength in cm, and c is the velocity of light in cm per sec. When $\sigma = 10^{-13}$, $\lambda = 10^5$, and $c = 3 \times 10^{10}$; $2\sigma\lambda c = 600$, and the earth behaves approximately as a perfect conductor. As regards the absolute value of the field intensity (Section 5), having satisfactorily proved that (in the case considered) the earth behaves as a perfect conductor, it is really unnecessary for the authors to show that the field intensity is just twice that which would be produced by the same oscillator isolated in space, as this fact must necessarily follow from the first. Uncertainties arise only when the earth has such a low conductivity that $2\sigma\lambda c$ is no longer large compared with unity. In discussing the use of the "image theory" in elucidating such questions the authors point out that this theory apparently gives rise to errors and inconsistencies. These appear to me to arise from a misuse of the image theory: it is not applicable to regions in the immediate neighbourhood of the transmitter. The original generalized image theory was first given, I think, in my paper on "Short-Wave Wireless Telegraphy."* In the derivation of the image theory from Sommerfeld's reciprocal theorem, it is pointed out that the theory only applies to points P at great distances from the transmitter T and sufficiently far above the earth's surface. This distance must be such that the field at T produced by a virtual transmitter at P is that due to an isolated doublet at P together with the reflected wave at T. This condition cannot be satisfied if P is close to T and on the ground. In the appendix to the same paper it is shown that Sommerfeld's analysis of the spreading-out of waves on an imperfectly conducting plane is in general agreement with the above, and that, in particular, there is a surface wave of the Zenneck type in the neighbourhood of the transmitter (not too close) which, together with the effect of the aerial and its image, gives the total field. There is thus no experimental or theoretical inconsistency with the image theory when this is properly used. The question whether Sommerfeld's analysis is applicable right up to the neighbourhood of the transmitter, is still open. Sommerfeld himself says that his theory is definitely not applicable to regions in the immediate neighbourhood of the transmitter, and only holds where the range is much greater than λ. The subject of the modification, both theoretical and experimental, to the simple Hertzian theory occasioned by the imperfect conductivity of the earth would seem to form a fitting continuation of the present investigation.

Mr. L. Ley K. Honeyball (*communicated*): The authors' work is a definite advance on some similar investigations in which Prof. Palmer and I were concerned about two years ago.† I consider, for instance, that their choice of the long wavelength of 1 000 metres is better suited to the elucidation of certain of the problems connected with the field strength at distances within one wavelength from an antenna, than were the wavelengths of 6 to 8·5 metres which we adopted. At the same time, when considering the optimum wavelength for our work I, for my part, had it in mind (perhaps quite wrongly) that for the study of fields at distances from the antenna of less than one wavelength, it would be better to work with short waves in order to make these distances so small that absorption and attenuation effects would be minimized. In addition, I considered that the use of short waves would render direct measurement of the received current possible, and so eliminate possible sources of error here. Furthermore, it becomes a simple matter when working with

* *Annalen der Physik*, 1926, series 4, vol. 81, p. 1139.

* *Journal I.E.E.*, 1927, vol. 65, p. 600.
† *Ibid.*, 1929, vol. 67, p. 1045.

wavelengths and distances of only a few metres, to obtain an uninterrupted optical path between the two aerials. At wavelengths of about 1 000 metres this may not always be easy, and in our own case it would have been impossible. However, the fact that the authors are able to obtain on a wavelength of 1 000 metres an almost uniform current distribution in their aerials is, as they have shown, so great an improvement as to outweigh any of the advantages which I have mentioned in favour of using short waves.

Messrs. **J. A. Ratcliffe, L. G. Vedy,** and **A. F. Wilkins** (*in reply*): The criticism of our attitude towards the previous measurements, as discussed in Section (2), gives us reason to believe that we may have been misunderstood in this respect. As mentioned in that Section, we realize that the previous measurements were made with the primary object of investigating the reflecting properties of half-wave aerials such as are actually used in practice, and from this point of view we believe them, with Dr. Smith-Rose, to be the best possible, and we believe that these previous workers have solved completely the problems which they set out to investigate. We, however, were interested in the validity of the Hertzian dipole formulæ, and on looking round to see whether they had ever been tested experimentally we found that these same experiments were the only ones which could be interpreted as a test of the formulæ. We were then forced to the conclusion that, from this point of view, the experiments were not quite satisfactory. There were small disagreements with theory in the work of Tatarinoff and of Palmer and Honeyball, which, although of secondary importance from the point of view of the reflecting aerial experiments, were yet sufficiently large to make it appear that there might be something wrong with the dipole theory.

Several speakers have referred to the measurements of the absolute value of the field, and some have suggested that it was unnecessary to measure this. Quite apart from all questions of theory, we thought it was useful to measure this field while we had the ideally simple aerial in operation, because we did not know of any experiments in which this had been done. It is clearly of extreme practical and theoretical importance to know whether the usually assumed value is the correct one or not.

As regards the theory, we are specially interested in the very helpful remarks of Mr. Eckersley. We were led to our point of view along the following lines. Suppose a small dipole A is put high above the surface of the earth and that the field due to it is measured at a point B on the surface of the earth. Suppose also that the distance AB is a few wavelengths, so that the waves incident at B are effectively plane. Then the field at B is that due to the plane incident wave together with the plane reflected wave calculated according to Fresnel's formula. Suppose now that the dipole A is lowered towards the surface of the earth; the reflected wave changes both in magnitude and phase as shown in Fig. F. This figure has been calculated for our case of $\lambda = 1\,000$ m, taking $\sigma = 10^8$ electrostatic units, and shows how the magnitude and phase vary with the angle of elevation (complement of the angle of incidence) of the dipole. If we continue to calculate according to Fresnel's formula we therefore reach the obviously incorrect conclusion that when the dipole is just above the surface of the earth the field at B is zero. Moreover, this conclusion is always reached provided the conductivity σ is finite. The case of a *perfectly* conducting earth is a limiting case which is *not* approached, for grazing angles of incidence, by taking the known values of σ. This presumably means that somewhere the Fresnel reflection formula breaks down; it is apparently valid when A is at a considerable distance above the earth, but it must not be used for angles of incidence which are too near grazing. We do not know under what conditions the formula ceases to give the correct value.

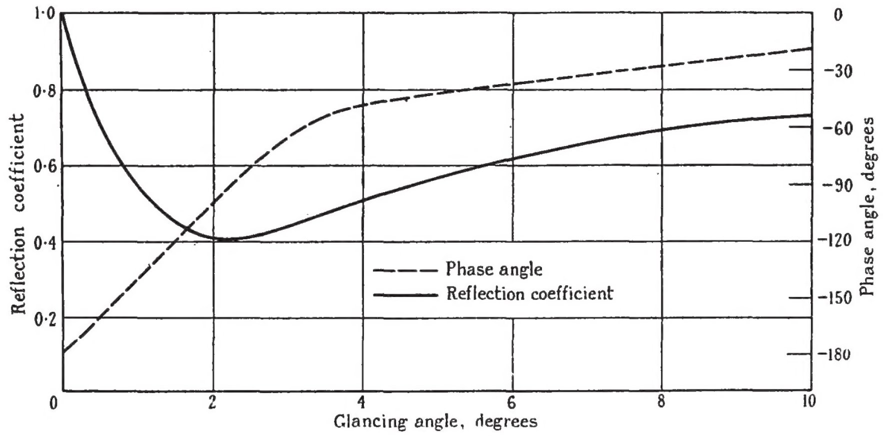

FIG. F.

Now these considerations may be of great practical importance. Suppose we have a high aerial (possibly on a hill rising from a flat plain) and we are working at a distance of 2 or 3 wavelengths from it, so that the waves from each element may be considered plane. Then we might try to estimate the intensity of the field due to it by integrating the effects of incident and reflected waves from a series of dipoles arranged along it. We might expect Fresnel's formulæ to hold for those elements near the top, but not for those near the bottom.

What contribution must we assume for the elements near the bottom? One method of procedure is to determine this experimentally by a method such as we have used, and then in our calculation to allow for some reasonable gradual transition between the measured value for elements near the base and the value calculated from Fresnel's formula for elements near the top.

We are especially indebted to Mr. Moullin for drawing our attention to his very elegant method of representing any current distribution by means of a series of dipoles. This method shows, much more simply than ours, that at sufficiently large distances the dipole formulæ should hold, and that the field is proportional to $\int i \, . \, dl$. It does not show, however, that at nearer points the field should vary with distance as given by writing r_1 in equation (9); in fact his argument, which builds up the actual aerial from a series of dipoles with their centres at the same point, would lead us to expect that the field would be equivalent to that from a dipole of the "mean" length, and hence that we should use some value of r intermediate between r_1 and r_0.

Referring to Fig. 10, we do not lay any great stress on the departure from the straight-line relation. It may very probably be due to the fact that the near-in readings were taken directly on a Moullin voltmeter across the aerial coil, so that the effective resistance of the aerial was increased, due to the grid current of the voltmeter, when the signal was strong. An effect of this kind would always produce an apparent "negative attenuation" effect on signals measured in this way.

Prof. Mallett's treatment of the subject is, of course, exactly parallel to the one given by us, and his graphical treatment of the reflecting aerial is exactly as explained by us in connection with Fig. 10.

Prof. Palmer refers to his assumption that the dipole formula may be used to represent the field from a half-wave aerial for the critical distances $r = 0 \cdot 33\lambda$ and $r = 0 \cdot 85\lambda$. In his paper, however, he also extends this assumption to the distance $r = 0 \cdot 16\lambda$ when considering the variation of current in the aerial B remote from the transmitter.* It is this assumption, in particular, which is criticized in our paper, and on referring to our Fig. 6 we find that the phase difference between a dipole and a half-wave aerial at this distance is about 40°. Even at $r = 0 \cdot 2\lambda$ it is about 20°. Phase differences of this order will make a great difference to the calculation of the current in aerial B in Palmer and Honeyball's case, but it is not worth while to calculate the effect here because we believe that at these very close distances, and with half-wave aerials, the analysis of Palmer and Honeyball is incomplete and account must be taken of the re-radiation from *both* the aerials A and B simultaneously in order to account properly for the observed maximum in current in aerial B at $r = 0 \cdot 2\lambda$.

Prof. Palmer refers to Fig. 14. We did not include values of θ in this diagram since, as explained in the text, they may be obtained from the values of the Moullin voltmeter readings which are plotted in the figure. Working in this way we obtain, from the small diagrams reproduced in the paper, the following values of θ at the maximum and minimum points respectively.

* *Journal I.E.E.*, 1929, vol. 67, p. 1047.

Distance		5 m	10 m	15 m
Prof. Palmer's values	Max.	−33°	−42°	−43·5°
	Min.	+57°	+48°	+46·5°
Values from Fig. 14	Max.	−30°	−43·5°	−43°
	Min.	+60°	+52°	+43°

We think that these agree with those calculated by Prof. Palmer as well as can be expected, considering the nature of the diagrams from which they are calculated.

Prof. Palmer asks about the tuning of aerial B. This was always tuned, but we cannot understand why its state of tuning should make any difference whatever to the field at B, since this aerial was intentionally made so small that any field re-radiated from it to A should be negligible compared with the main incident field. For the same reason it does not appear to us correct to say that the tuning of B determines whether A or B carries the larger current. This is determined by the resistances and heights of the two aerials, which are completely different. We cannot understand why this consideration should be taken into account when checking the ordinates of Fig. 14.

Prof. Palmer refers to the effect of neglecting the angle η. As pointed out in the paper, the effect of this is most clearly seen from the circle diagrams. We have constructed such diagrams to correspond to Fig. 15(*c*) with the main field vector OA displaced through 3·5°, first in one direction and then in the other, to correspond to the two extreme cases when aerial B (Fig. 12) was directly behind aerial A, and when it was directly in front of A. As Prof. Palmer points out, the change makes most difference to the point lying on OB, and a small difference to the point lying on the line between OB and OC, but the change in the other points is inappreciable. We believe that any difference between the three results was beyond the range of accuracy of our experiments, because we always found substantially the same curves for all relative orientations of aerials A and B. We agree with Prof. Palmer that this inability to follow the variations of phase with distance is a great weakness inherent in the use of long waves for shadow-aerial experiments.

Prof. Palmer also refers to the apparent variation of the effective height h' when calculated back from the results shown in the table on page 534. The "calculated" values of α in this table were obtained by using the value of $h' = 3 \cdot 35$ m and $h = 4 \cdot 15$ m, and this gives directly the values of α for the distances 15 m and 10 m. For the closer distance we did not use the value of r_1^3 directly, because the results shown in Fig. 10 led us to suppose that the r_1^3 law was not exactly obeyed here. We therefore corrected the value obtained, by a factor deduced from the curve of Fig. 10. If this correction is not applied we obtain the value $1 \cdot 02$ for α at this distance. As previously explained, we are now a little doubtful about the reality of the drop in the curve of Fig. 10, and so perhaps this latter value is the more exact one.

We cannot agree with Mr. McPetrie that most of the

radiation from an aerial takes place from the free end. The moving electrons all along the wire must be responsible for the field produced. In order to calculate the total effect of the radiations from all the electrons we should have to perform an integration up the wire and then we should find an expression like (9) in which the important distance r_1 is to be measured to the free end of the wire. This shows that the field is determined by the distance to the free end of the wire, but this is quite different from stating that all the radiation takes place from the free end. If it were true that all the radiation takes place from the free end of the aerial, then Mr. McPetrie's own method of calculation, by integrating the effect of a series of dipole sources up the length of the aerial, and ascribing to each source its proper reflection coefficient at the ground,* would not be correct. If the reflection coefficient from the ground had been different from unity then the shape of Figs. 7 and 9 would not have been altered, provided the reflection coefficient did not change with the distance from the transmitter. The only way in which we might expect it to change would be in virtue of the fact that the angle of incidence from the higher portions of the aerial changes as we get nearer. We do not agree that it is redundant to measure the magnetic field when once the variation with distance of the electric field has been measured. The value of curl E depends not only on the variation with distance of the vertical component of E but also on its phase variation and on the horizontal component of E. Dr. Smith-Rose has emphasized this in his remarks and we entirely agree with him. It was exactly for the reasons stated by him that we decided to measure both H and the vertical component of E, but we realize that these two measurements do not completely determine the field. We cannot trace in the paper the " explanation " to which Dr. Smith-Rose takes exception.

We do not agree with Mr. McPetrie as to the limitations to the application of Fresnel's reflection formulæ. The formulæ certainly *do* apply only to plane *incident* waves, but we do not consider that there is any restriction to the case where we are so far from the individual scattering electrons that the secondary scattered waves due to these should be plane also. We believe that the expressions are true even just outside the reflecting surface, where some of the secondary scattered waves

* *Journal I.E.E.*, 1932, vol. 70, p. 382.

(those from near the surface of the reflector) will have a small radius of curvature, but they will all envelop a plane reflected wave. The question of the combination of the secondary scattered waves to give the resultant reflected wave is dealt with in a very helpful paper by Prof. C. G. Darwin.* In this paper he assumes the reflected wave to be plane right up to the boundary surface, and shows that it satisfies the equations obtained by adding together all the scattered waves. Furthermore, if Mr. McPetrie's suggestion were valid it would make Fresnel's expression for the *refracted* wave quite meaningless, for if we are inside the refracting material we can never be so far from all the scattering electrons that their secondary waves appear to be plane. We are also unable to agree with Mr. McPetrie's suggested explanation of the reversal of the phase of the reflected wave. The scattered waves which combine to make up the reflected wave near the surface of the earth do not come entirely from nearby points. They come from oscillators at some considerable depth in the earth and, more important, from oscillators near the surface but some distance away. We cannot say without working the case out in detail what the resultant effect will be. We cannot do better than quote Prof. Darwin. " All we can say is that the relation between scattering and refraction is a complicated relation, that it is possible to deduce the one from the other quite correctly without deeper inquiry into the mechanism of scattering, but that the interrelation is so involved that it can only be treated by mathematical methods and that the methods of direct intuition are as likely to be wrong as right." Darwin's calculations take into account the total (radiation + induction + static) field of the scattering centres and, as previously mentioned, he does not find that the reflected wave departs from the Fresnel form.

We are very pleased to see Mr. McPetrie's paper, to which Mr. Barfield refers, which gives such a neat explanation of the short-wave shadow-aerial results. The agreement between the new theory and the experiments is a testimony to the accuracy of the latter. We are very interested in Mr. Barfield's new explanation of the " town shadow " effect, and we look forward to seeing it worked out in detail.

We refer under " Discussion of Results " on page 530 to the effect of the flat top of the aerial, mentioned by Mr. Turner and Dr. Rayner.

* *Transactions of the Cambridge Philosophical Society*, 1924, vol. 23, p. 137.